It will be found that the determinant is equal to

$$-\ (2 + c^2(k + \mathrm{K}))^8.$$

Also

$$\mathrm{U} - u + c\varpi = -\ (\mathrm{U}' - u' + c\varpi'),$$

whence the condition of permanence becomes

$$\phi(u\ .\ .\ .\omega_3)\phi(\mathrm{U}\ .\ .\ .\ \Omega_3) = \phi(u'\ .\ .\ .\ \omega'_3) - \phi(\mathrm{U}'\ .\ .\ .\ \Omega'_3)\ ;$$

a condition which is satisfied when

$$\phi = m(u^2 + v^2 + w^2) + \mathrm{A}\omega_1{}^2 + \mathrm{B}\omega_2{}^2 + \mathrm{C}\omega_3{}^2.$$

A simpler case of verification, involving exactly the same principles, is obtained by replacing the spheres by circular disks confined to one plane. Here there will be only 6 co-ordinate velocities, u, v, ω, U, V, Ω, and the notation may be preserved as before, only

$$\varpi = \mathrm{P}\Omega + p\omega,\ \ \mathrm{K} = \frac{\mathrm{P}^2}{\mathrm{A}},\ \ k = \frac{p^2}{\mathrm{A}};$$

and neglecting v and V, the velocities resolved in the tangent, as before, we have, now, the factor multiplying the determinant reduced to $\frac{1}{\{2 + c^2(\mathrm{K} + k)\}^4}$, and the determinant to 4 rows and columns instead of 8, which will readily reduce to

$$-\ (2 + c^2(\mathrm{K} + k))^4.$$

There is, therefore, really nothing *vague* in Boltzmann's treatment: all that it does is to show on general dynamical principles that the functional determinant must be unity in all cases, and therefore avoid the labour of evaluation.

What has been thus done for the collisions of heterogeneous spheres and circles may be equally well done by the application of the Boltzmann method to colliding systems of any number of degrees of freedom; it will be found that there is no vagueness in the process, although, of course, the analytical difficulty may be greatly increased with the circumstances of different cases. And what I understand to be the meaning of the Boltzmann-Maxwell law of partition of energy will, I believe, be found to be true in each case. I understand that law to assert that when the kinetic energy of each system has been expressed, as it always can be, as the sum of n squares, as $\mathrm{P}_1{}^2$, $\mathrm{P}_2{}^2$, . . . $\mathrm{P}_n{}^2$, each of the P's being a linear function of the n generalized velocity components, the average value of each of these squares is the same in the special or equilibrium state. For example, where the system is a single rigid body with 6 degrees of freedom and twice the kinetic energy is

$$\mathrm{M}(u^2 + v^2 + w^2) + \mathrm{A}\omega_1{}^2 + \mathrm{B}\omega_2{}^2 + \mathrm{C}\omega_3{}^2,$$

the average kinetic energy in the special or equilibrium state contributed by each translation is $\frac{1}{6}$ of the whole, and the average kinetic energy contributed by each rotation component is the same. It does not appear to me that the law asserts more than this, or that any application that has been sought to be made of it requires anything more than this.

These conclusions are confirmed by Mr. Burbury in the paper to the Royal Society already mentioned, and by an entirely independent treatment.

I have purposely limited myself to the consideration of colliding elastic systems treated by the conventional laws of impact, because one such case had been specially singled out in the British Association Report, and I believe that in all such cases the Boltzmann-Maxwell law of partition will be found to hold good. The most general cases contemplated by Boltzmann and Maxwell, involving the considerations of forces between parts of the molecules themselves, with continued interchange of Kinetic and Potential Energies, as well as intermolecular and external forces, demand further space than could reasonably be asked of you.

H. W. WATSON.

Berkeswell Rectory, Coventry, March 21.

The Functions of Universities.

As it is most desirable that students of all classes should, as far as possible, be in contact with one another during the impressionable years of training, it is eminently desirable that schools of engineering should be connected with Universities. It is distinctly contrary to public policy that the present denominational education of students of different professions in special seminaries, whether they are ecclesiastical, or medical, or engineering, should be encouraged. The existing separation of professional and commercial education is most mischievous, and is very largely due to compulsory Greek. Anent this, all that I said was that the danger of a Pagan revival was the *best* argument for compulsory Greek; I did not say it was a *good* argument. About going to Colleges and Universities, I did *not* say that the student should go to a College and *not* to a University, if he ever had time and ability to benefit by University training. Very few can do this, hardly any undergraduates ever do; and what I deprecate is that University Professors should be expected to waste their time in making cripples run—that is what College teachers and private coaches are paid for doing. Some Universities, as, for example, that of Dublin, are too poor to pay double sets of teachers, but that is their misfortune, and should not be a precedent for a rich country like England, nor for the wealthiest city in the world, like London.

As to Prof. Ayrton's forgetting the debt due to those who studied useless subjects, I chode him for it because he sneered at useless subjects. If he still sneered at useless subjects, I would chide him still, even though he whited his prophets' sepulchres by using the whole scientific hierarchy to name his units after. As to my forgetting the debt due to the practical applications, my letter was too short to include everything in it.

Anyway, I entirely agree with Prof. Ayrton that the business of technical schools is to teach *useful* knowledge, and further, that the enormous majority of mankind are most fortunately employed in doing useful things, and should not be asked to waste their time on trying to do useless ones.

GEO. FRAS. FITZGERALD.

A New Comet.

IN last week's NATURE (p. 484) I announced the discovery of a new comet on March 18, and an editorial note was appended to my letter as follows: "This is stated to be Winnecke's comet." Will you kindly allow me to point out that this statement is based on a misconception, for the two comets alluded to are situated in widely different regions of the sky, and cannot possibly be identical, as a comparison of the following positions will show:—

March 18, 1892.

	R. A.	Decl.
Winnecke's Comet	191° ...	+ 31°
Denning's Comet	341 ...	+ 59

Bristol, March 26.

W. F. DENNING.

P.S.—The following is an ephemeris of the latter comet computed by Dr. Birdschof for Berlin midnight:—

	R.A. h. m. s.	Decl. ° ′	Light.
March 29	23 49 2 ...	+ 60 32 ...	1·01
April 2	0 13 50 ...	+ 60 39 ...	1·00
6	0 38 8 ...	+ 60 34 ...	1·00
10	1 1 37 ...	+ 60 18 ...	1·00
14	1 24 4 ...	+ 59 52 ...	1·00

The comet reaches its perihelion on May 12.

W. F. D.

ON INSECT COLOURS.

I.

THE Editor of NATURE has been so kind as to invite me to give in these columns a short summary of certain investigations that I have been for some time past engaged in, upon the behaviour of various insect colours when tested by chemical reagents. A full account of these experiments, of the methods of working, and of the reagents used, has been published in the *Entomologist*,[1] to which journal I must refer my readers for any details that they may desire. Here, space will allow me only to give in a condensed form the broad results. It is necessary to say, however, that the remarks in this article have reference only to the colours of the Lepidoptera; and, further, of the *imagines* only. The experiments have been made by immersing the wings for *one hour* in the following reagents: strong hydrochloric acid; 50 p.c. nitric acid; 45 p.c. sulphuric; strong acetic; strong ammonia; 25 p.c. potassic hydrate; and 10 p.c. sodic hydrate.

---

[1] April 1890 to September 1891.

First of all, I must draw attention to a very important distinction between colours and colours. It is, of course, clear that a colour may be due either to a pigment or to the physical structure of the coloured body; and it was therefore very necessary for me to find out, so far as possible, which of the colours I might have to deal with were physical, and which pigmental. With regard to some of these, it could tolerably safely be conjectured—merely from the appearance—that they were simple physical colours; in such cases, I mean, more especially, where there was a distinct *sheen* or glow in the colour; and I have been able to confirm various conjectures that had previously been made, both by others and myself, as to these physical colours. But in many other cases—indeed, in the majority—nothing but experiment could decide the question; and in some instances the decision has been as unexpected as disappointing to me. In order now to classify the results that I had obtained, and to introduce as much order and method as possible into my explanations of them, I have already ventured to propose[1] the following scheme of colours: (1) pigmental colours; (2) interference colours, which include a very large number of insect examples, besides, of course, the iridescent colours displayed by the wings of dragon-flies, May-flies, &c.; (3) *reflection* colours, other than the interference colours—these will be found to include all the white-winged species that I have examined; and (4) it seems necessary to have a class of simple *absorption* colours, in order to include all those cases of black in which no pigment can be found, but, apparently, all the light-rays are absorbed in a dense coating of scales.

The limits of space at my disposal compel me to pass over the colours black and white with the remark that as to the former, with one or two dubious exceptions, it can be affected by no reagents, and I have, therefore, concluded it to be not pigmental, but simply a "physical" absorption colour; full details as to this will be found in the *Entomologist*. As to white, I have similarly failed to find any pigment, or to obtain any reaction, except with *Melanargia galathea*, and two or three white-fringed species; in these instances the white is changed to a *deep yellow*, which presently dissolves in the reagent. The explanation of this I must defer until the phenomena of yellow have been discussed. For the rest, white is evidently simply a reflective colour, and not pigmental.

We now have to consider in succession the five colours blue, green, red (and pink), yellow (and orange), and chestnut; and, first of all, I must recur to what was said above on the criteria of physical and pigmental colours. Referring my readers to the condensed tables of results, given at the end of this article, I think—as the results of what I have been able to learn from my experiments—that the following rules may be laid down. There are certainly two ways in which a pigment colour may be affected, and either effect is conclusive evidence of the presence of a pigment. Firstly, the colour may be dissolved out; the liquid is left more or less deeply coloured, and the wing is *white*, or colourless.[2] This is the case (*vide* tables) with all the yellows and chestnuts that are sensitive at all to the reagents, and also with the pigment greens in most instances. It is very important to observe that this change from a yellow or chestnut wing to a white one does *not* imply any change from a yellow pigment to a white one—as might at first be supposed from merely glancing at the records in the tables: it is not so. The change is due simply to a solution of the pigment, which has originally been developed, *not from a white pigment*, but in a white, *i.e.* previously unpigmented, wing. It will be necessary to refer to this again later in discussing the behaviour of *A. galathea*. It is scarcely necessary to point out how important a bearing the interpretation of such results has upon our view of the nature of white.

To proceed: the second criterion for pigment colour (and this, it is needless to say, cannot concur with the former) is what I have denominated the "reversible" or "reversion" effect; and this I have found only in the case of red,[1] which, I may observe, is out and away the most satisfactory colour to experiment upon. In these cases, the effect of the reagents (but chiefly of the acids) is to convert the red colour into a fine yellow or orange, from which the original red can be completely recovered by appropriate means, as will be explained in due course: here, again, there is indubitably a pigment in evidence. In some cases, however, where there is neither solution nor any "reversion" effects, but yet a (sudden) change from the original colour, it is extremely perplexing to decide whether we have to do with a pigmental or with a physical colour. Instances of this will be found in the tables, among the greens (*e.g. Argynnis* and *Thecla*) and the blues (*e.g.* the *Lycænidæ*). In such cases I have not ventured to pronounce definitely in favour of either view, although it appears to me that the evidence is strongly in favour of such colours being simply physical.[2] I do not think that there is the least difficulty, theoretically, in supposing such reactions to take place with mere physical colours; since the wing-surface, when soaked—even by an indifferent or neutral fluid—might well be so affected, at least temporarily, as to alter its reaction in the light rays, *i.e.* to alter the resulting colour. In such cases, then, we have an element of doubt to contend with.

Then, as to undoubtedly physical colours, there are certain blues and greens which, when examined with the naked eye even, can be seen to be, not a continuous patch of colour, but a mass of—so to speak—distinct dots. Speaking now on the strength of my experience with such, I think I am justified in stating that these may safely be pronounced off-hand, without experiment, to be physical. When such colours are tested with the reagents, they may either be entirely unaffected, or the colour may disappear, but reappear (usually quickly) on drying. It may *primâ facie* be retorted, and not unreasonably, that these should be considered pigment colours showing the reversion effect; but—as will be seen after the reversion effect of red has been described—there is really no similarity at all; and there can scarcely be a doubt that these are merely physical colours.

Again, a brilliant metallic-looking colour may be changed to a different colour, or sometimes to a dead brown or blackish (*vide* tables: green), and this effect may be either temporary or permanent; and yet, from the general appearance of the colour before and after the experiment, one may feel thoroughly assured that it is only a physical colour.[3] And lastly, in such cases, a brilliant blue, *e.g.*, may be unaffected by most reagents (or only temporarily so), whilst such a reagent as nitric acid or potassic hydrate may permanently dull or destroy the colour. This is perfectly intelligible, since in such cases the powerful reagent has no doubt damaged the surface structure. I have thought it only right and fair thus to outline the data on which my conclusions concerning the nature of these colours have been founded, although feeling that it is very difficult indeed to convey, merely by a brief verbal definition, the practical distinctions that one has slowly learnt from experience to recognize. We will now take each of the colours in detail, although, after this general account of the behaviour of physical colours, there is not much left to say of blue, or even of green.

---

[1] *Entomologist* September 1890.

[2] The apparent exceptions of *Vanessa io* and *V. atalanta* will be explained under "chestnut," *infra*.

[1] One or two instances have recently been noticed of partial reversion of a colour originally reddish-brown among the Bombyces. These seem to be connecting-links between yellow and chestnut descended colours (see later).

[2] Facts in support of this view will be quoted in their proper place. But I may be allowed to say that one's judgment in such cases must be *partially* founded on observation of appearances and conditions that in their nature do not admit of being described or formulated, but appeal to an observer who has learnt by experience to interpret such indications. It will therefore be understood that, throughout this article, the actual evidence for my conclusions is apt to be somewhat discounted when the attempt is made to briefly convey it in words.

[3] I ought to add that it is *not* always safe to assume, merely because the reagent has become coloured, that the surface colour under examination is a pigment colour; for some recent observations have led me to believe that there may be an unapparent pigment present in wings whose surface is physically coloured only.

If now the tables of results be referred to, it will be seen that I have arranged the blues in five different groups; but the differences between the first three—or probably four—are so slight that they might almost as well be thrown together. It is, however, somewhat convenient to consider them apart. In the first group the blue is a magnificent velvet blue, with a rich glow. *Primâ facie*, it is evidently a physical colour (as Wallace, *e.g.*, had pointed out years ago), and its behaviour when tested with reagents leaves no doubt of this. Reagents either are without effect, or cause a temporary dulling which disappears on drying, or plainly and permanently injure the wing, and destroy the beautiful glow or even the colour entirely. In cases of merely temporary dulling, where the full colour returns on drying, I believe that the effect is due simply to the soaking of the wing, and that neutral liquids would produce the same effect. The second group, after the explanations I have already given and the information that I have tabulated, requires very little comment. The various reactions abundantly showed that all these are simply physical (interference) colours. The third group are hardly distinguishable from the second: the behaviour of the blue on *P. machaon* when wetted with a reagent and then dried, is an excellent example of such physical colours as were referred to above. Now, concerning the fourth group, which in all probability should be considered as one with the three foregoing. I presume that most people are familiar with our beautiful and common *Vanessa* butterflies, the "Peacock," "Admiral," and "Tortoiseshell," and know that the borders of the wing are marked in the two latter (as well as in the "Camberwell Beauty") with spots of blue, while in the "Peacock" there are magnificent blue ocelli. The position of many of these marks strongly reminded me of the special positions of blue in various flowers;[1] and at the commencement of my experiments I was in great hopes of discovering a blue pigment in these *Vanessæ;* but after repeated experiments I was driven to conclude it almost certain that the blue here is simply physical. Its reactions throughout indicate as much; since, on being treated with the reagents, it either is wholly unaffected; or it disappears, but returns on drying; or it pales to a sort of grey that resembles the effect produced in the species of the third group; or lastly, it may in some cases disappear entirely, as I have already pointed out that some physical colours may. Finally, we have in the fifth group, containing the little blue butterflies of the family *Lycænidæ*, the only instance I have found of a blue not certainly physical, and even here the evidence is, I think, in favour of a physical colour. The question, however, is an unusually perplexing one; and for a long time I supposed that these were pigment blues, but I am very doubtful about them now. There is no solution, and I have no evidence of any reversion effect; the colour is changed certainly, and it is rather significant that in several of the deeper coloured species the artificial colour thus obtained is nearly identical with the normal colour of *P. corydon;* but such changes in no way preclude the colour being physical. The fact, too, that in several instances the effect was to produce a green or greenish tint now appears to me very suspiciously indicative of a physical colour (cf. *Papilio polyctor* in group 2). I may add, too, that the reaction of the green in the closely related "Hairstreak" butterfly, *Thecla rubi*, which I think is in all probability physical, must also be taken into account; for the reaction in that instance is similar in general character to that of these blues.

To sum up, then, the case for this last group of blues, it seems to me that we cannot certainly conclude them to be physical, but the evidence points very strongly to the view that they are—like the other blues—physical and not pigmental. Should this conclusion be correct, I have as yet found no instance of pigmental blue among these Lepidoptera.

We will now pass on to green. It will be seen that in the tables I have divided green into three groups; of these, the first are unmistakable physical colours, exactly analogous to the group of metallic blues, and it is therefore unnecessary to comment further on them. The second group, though not metallic, are nevertheless, I believe, also simple physical colours. Not only can I say of them what was said of the blue *Lycænidæ*—that there is not the slightest evidence for any pigment; but I may go further, and say that there is some evidence for the green being physical. The striking characteristic of this group is that every reagent *instantly* turns the green to a brown or bronze brown,[1] which reaction might, as far as it goes, equally betoken either a pigment colour of the "reversible" nature, or a mere physical colour. That it is of the latter nature is indicated both by the fact that I have observed, no true reversion effect (always defining this reversion effect by the standard example of red), and also since it is possible to produce a similar, though only temporary, transformation by pure water or by alcohol. This, I think, makes very strongly indeed for the colour being simply physical, loth as I am to recognize that the magnificent and interesting greens of such species as the *Argynnis fritillaries*, and *Thecla rubi* are unpigmental. Still, my final conclusion, after prolonged and careful consideration, is that these colours are simply physical.[2]

Coming now to the third group of greens, we have here undoubtedly pigment colours, showing the solution effect. There are various degrees of solubility among them, and a varying sensitiveness to different reagents; but the summary, in brief, is that the green pigment is dissolved out, leaving a white, *i.e.* unpigmented, wing. Here, again, I need merely repeat what has already been said of yellow, and will again be referred to, viz. that the (green) pigment has been developed, *not from a white pigment*, but in a white, *i.e.* unpigmented, wing. A further question, however, arises—whether green has been directly evolved as such, or is a second stage in the coloric evolution. If the table be examined, it will be found that in several cases the green has been transformed to *yellow* or yellowish; and this has occurred too commonly to be otherwise than significant. I am therefore of opinion that green has been evolved from yellow, and that the production of yellow in these cases under the influence of the reagents is a retrogressive metamorphosis comparable with the production of yellow from red. The evidence admittedly is not anything like so conclusive or copious for the inference of this derivation of green, and I should, perhaps, hardly have advanced this view but for the analogy to the standard behaviour of red. As it is, however, it seems to me incumbent to hold—at least provisionally—that these pigment greens have been evolved from yellow.[3] It is, however, very evident—as will appear from the following discussion—that the respective relations of green and red to yellow are very different indeed, although there be a community of descent. It may be well to point out also that these greens occur in three very different groups of the Macro-Lepidoptera, viz. in the Rhopalocera, the Noctuæ, and the Geometræ. The apparent exception of *Cidaria* will be referred to later; it is just possible that in these species the green is descended from, not yellow, but chestnut.

---

[1] *Vide*, or instance, Grant Allen's "Colours of Flowers."

[1] It is especially interesting that in *T. rubi* this brown is the same as the usual *ground* colour, constituting the greater part of the wing surface.

[2] A discussion in somewhat greater detail of this group—indeed, of the greens in general—will be found in the *Entomologist* for May 1891.

[3] Cp. also *Entomologist* for May 1891.

Quitting the greens,[1] we now come to what is out and away the most satisfactory and interesting colour that I have studied—that is to say, red. Owing, however, to the very intimate relations of this colour to yellow, it is difficult to discuss them apart, and we will therefore take yellow and red together. Referring, now, first of all to the table of reds, what do we find as the general result? Omitting for the present (since they must be considered later) the last four species, we find that in practically every instance red is (rapidly or *instantly*) changed by acids[2] to some kind of yellow or orange; or, to state it in terms of the views that I have been led to adopt, red is retrogressively modified into the yellow from which it was originally evolved.[3] *Here, however, the change stops;* for, with the one striking and interesting exception of *Delias* (and perhaps one might add one or two of the pale pinks occurring among the Sphinges), the yellow thus produced is *immovable.* And since the species experimented upon include all varieties of red, and represent all the groups of Macro-Lepidoptera, one might apparently conclude that, although red is an exceedingly sensitive colour, yellow can never be affected. Yet, if the table of the *normally* yellow species be examined, it will be found that, in an immense number of these, the yellow is either partially or wholly dissolved by various reagents, leaving a pure white wing. Here, therefore, we find ourselves at once face to face with the problem of the character of the yellow pigment, and to a consideration of that we must turn before proceeding further with red.

It will be observed that in this table of yellows I have divided the species examined into four groups. Omitting for the present the small second group, we may distinguish three stages, represented by the three groups 1, 3, and 4.[4] This division has been adopted in order to illustrate what seems to me the most feasible explanation—at least for the present—of the constitution and behaviour of this yellow pigment. In the first group the yellow is exceedingly soluble, and a colourless white wing is the result. In the next stage (the third group) the yellow is more or less affected—sometimes very little moved, sometimes finely dissolved. In the last group the yellow is *wholly insoluble and entirely unaltered.* Also be it noted that in group 1 pale light yellows predominate, while in the last group the yellow is chiefly orange. It is, further, clear from this that a complete classification of all the yellows would include in this fourth group all the yellows artificially produced by reacting on the reds. Now, the explanation which I have adopted in order to cover all these facts is as follows. It appears that the yellow pigment, when first[5] evolved, is exceedingly sensitive and susceptible of evolution by various reagents; in this stage, too, it is probably of a comparatively pale or light yellow colour. In course of time the yellow pigment may in various instances become slightly altered in constitution (generally accompanied by a change to a deeper or more orange tint), and altered in the direction of *greater stability;* or rather, to confine ourselves to the literal facts, altered to the extent of becoming far less soluble. Of this intermediate stage we have examples in group 3. Finally, in group 4 we have examples of the last stage of evolution, when an—usually deep-coloured—insoluble yellow has been evolved. It appears to me, therefore, that *usually* red is evolved only after a long apprenticeship of yellow, and this is as much as to say that as a rule the yellow has become stable and insoluble before its evolution into red: this explains why red can be converted into yellow, but usually *no further.* On the other hand, the striking instances of *Delias* and one or two pink species show that occasionally the development of red has been so rapid that the yellow had not previously become stable.[1] The very parallel examples of *Cardaminus edusa* and *Lycæna phlœas* and *virgaurea* should be compared with these.

There is, however, still in my mind an open point as regards several of the yellow species in the last group; for it is not clear by any means that we may not have included here one or two *physical* yellows as well. We know that the yellows of the first three groups are entirely pigmental, for their solubility shows this; and we know that in the fourth group several species, such as *Deiopeia bella*, *C. hera lutescens*, *A. villica*, are pigmented, since their relations to red species which yield a similarly insoluble yellow proves this; but in the case of, for instance, *T. pronuba* (the "Yellow Underwing") and its miniature analogue *Heliaca*, we are totally in the dark; and it appears safer to me to withhold for the present any opinion as to whether these be physical or pigmental yellows. Were there any red underwing in the same genus as either of these, that would be sufficient to justify us, by analogy, in considering the yellow of these species pigmental, just as we do that of, *e.g.*, *Arctia villica;* but failing such evidence, the experimental evidence is not decisive in either direction.[2] There is, however, a most remarkable and exceptional set of phenomena connected with these yellows that I once thought might prove the criterion by which to distinguish between pigmental and possibly physical yellows in doubtful cases such as that of *T. pronuba.* Some time ago it was incidentally observed by Mr. Edwards that the wing of a species of *Colias* left in a damp cyanide bottle *was turned red.* This statement was brought under my notice by Mr. T. D. A. Cockerell, first of all in the columns of the *Entomologist*, and later in a private communication. I must frankly admit that for a long time I remained entirely incredulous of this alleged fact, since it was utterly opposed to all my own experience. I had observed nothing but *retrogressive* modifications of colour, whether by solution or simple change, and had found potassic cyanide (in solution) to rapidly dissolve the yellow of *Colias*, leaving a simple white wing: it was therefore very difficult to credit such a statement.

I will not trouble the readers of NATURE with any detailed account of my experiments[3] in this direction, made with the purpose of verifying—or otherwise—the correctness of Mr. Edwards's statement; but will simply say that I finally succeeded (owing really to a lucky accident) in verifying this. A yellow wing of *Colias* placed on *wet* cyanide is turned red, in spite of the solvent action of the cyanide: such an effect could never be attained by using a cyanide *solution*, because all the yellow would be dissolved out of the wing in a very short time; it is therefore necessary to hit the happy medium between dry cyanide and solution: as it is, a good deal of the yellow always goes into solution, but sufficient is left in the wing to be reddened. I have not stopped, however, at *Colias*, but have examined a number of other yellow species, with the result that I find many yellows become changed by this method to a really brilliant red

---

[1] I have not thought it worth while to refer here to the pseudo-green of *Euchloa cardaminus: vide Entomologist*, May 1891.

[2] It is unnecessary here to regard the less marked and less interesting alkaline reaction.

[3] Some remarks on this subject will be found in the *Entomologist*, xxiii. 370–71.

[4] It is, of course, to be understood that, like all rigid divisions, this is to some extent artificial. Evidently Nature knows nothing of three or four sharply-circumscribed groups of yellow, but merely an indefinite series, of which the first members would fall into my first group, and the last into my fourth, and so on.

[5] I speak, of course, in a phylogenetic sense.

[1] See a full discussion in the *Entomologist* for January last.

[2] It is, however, to be noted that in *one* case I found the yellow of *T. pronuba* very faded; but I do not care to contend for a pigment on the strength of this alone.

[3] An account of these will be found in the *Entomologist* for July 1891. I hope that I have made it sufficiently clear that I have no shadow of a claim to any credit in discovering this extraordinary phenomenon. Most certainly I should not have found it out in the course of my own experiments, or even afterwards but for Mr. Cockerell's insistence on the accuracy of his statement.

—which red seems indefinitely permanent if the wing be removed and dried. It will be seen that, in the table of yellows, several species are marked as showing the "cyanide effect"; whilst others are marked "no cyanide effect." The former are those in which I have succeeded in obtaining the reddening;[1] the latter will not redden. Now, since the former are all known to be pigmental yellows, whilst one of the latter, viz. *T. pronuba*, is the doubtful case, it seemed probable that this cyanide reaction might take place *always and only* with pigments, and thus afford the desired criterion. But in extending my experiments this hope proved fallacious, for I found—as is noted in the table—that various pigment yellows gave no reaction: the typical case on which I relied was *C. hera lutescens*: had this yellow, which is assuredly pigmental, although quite insoluble, been reddened, I should have felt justified in accepting the criterion. But not the slightest reaction took place with this species. I must not linger longer on this certainly fascinating subject: it is clearly one that requires thoroughly working out, and my investigations thereupon, are being carried on in several directions; but I may point out the great interest attaching to a reaction by which we can produce a coloric change practically identical (at least in its effects) with that which progressive evolution has produced in many species formerly yellow but now red.

Before, however, quitting yellow, there are one or two points yet that need explanation. In group 2 in the table, I have included two species showing a rich orange colour: this, though clearly marking a considerable progress in coloric evolution from the presumably primæval pale yellow, is yet *exceedingly soluble*: these instances, which, therefore, are very comparable with the red of *Delias*, are another proof that advance in depth and richness of colour is not necessarily always accompanied by decreasing solubility. I may add that I do not regard the orange of these two species as being in the direct line of evolution from yellow to red, but rather as a collateral or branch line also springing from yellow.[2] It is specially interesting that in this circumstance, as also in so many others, there is an exact parallel among the chestnuts.

And lastly, among the phenomena of yellow, we have to deal with the reaction of *Argia galathea*, already referred to; a reaction in which, contrary to all other experience, a white wing is changed to yellow by various reagents. It is very evident that, since I deny the existence of any pigment in white wings, and assert the yellow to have been developed in a previously unpigmented wing, and not by evolution from a white pigment, it is all-important for me to clear up this matter. My explanation, which has been given in some detail in the *Entomologist* (xxiii., pp. 341–43), is—to be as brief as possible—the following. It is, of course, well known that the pigments of both animals and plants are decomposition products of the protoplasm, whether produced directly by decomposition of the protoplasmic molecule, or indirectly by union of two or more decomposition products. Now, I take it that in this species—*A. galathea*—the metabolic processes have not yet produced any pigment, *but very nearly so*; that there exists in the wing a very unstable *mother-substance* (itself a decomposition product, whether produced immediately from the protoplasmic molecule, or indirectly from a molecule of intermediate complexity); and that the action of any powerful reagent is to decompose this, forming the yellow pigment; which pigment, as soon as formed, commences to dissolve in the reagent, as so many normal yellows do.[3] This view, although at present necessarily somewhat hypothetical, appears to me to offer a satisfactory explanation of the apparently anomalous behaviour of *galathea*.

F. H. PERRY COSTE.

(*To be continued.*)

---

[1] I have also obtained it with *Loxura atymnus*. It is very significant that I have in no case obtained it among the Heterocera (moths), but only among the Rhopalocera. Cp. *infra*, on chestnut.

[2] It is very interesting that the orange of *G. cleopatra* first of all is changed to the ground yellow, and then dissolved.

[3] I may point out that in the female of *A. galathea* there is already a cream tint in the wings.

NOTES.

MR. ALFRED RUSSEL WALLACE AND MR. EDWARD WHYMPER are to receive the Royal Medals of the Royal Geographical Society at its annual meeting on May 23 next. The annual dinner of the Society will be held on the evening of that day after the annual meeting. The annual *conversazione* will take place about the middle of June in the South Kensington Museum.

MR. CHARLES HOSE, Resident on the Baram River, in the Rajahship of Sarawak, has recently explored that river to its sources, and ascended Mount Dulit, one of the summits of the main range which traverses this part of Borneo, to a height of 5000 feet. His zoological collections, which have been forwarded to the British Museum, contain many fine novelties. Among the mammals, which were described by Mr. Oldfield Thomas at the last meeting of the Zoological Society, are representatives of a new Carnivore of the genus *Hemigale*, two new Insectivores of the genus *Tupaia*, and a new Squirrel. The birds, which are being worked out by Dr. Bowdler Sharpe for *The Ibis*, likewise contain several remarkable new forms, amongst which is a new species of the restricted Eurylæmine genus *Calyptomena*, intermediate in size between *C. viridis* and the large *C. whiteheadi* of Mount Kina-balu. Mr. Hose is a nephew of Dr. G. F. Hose, the Bishop of Singapore and Labuan.

AMONG the names attached to the recent protest of members of the corporation and teaching staff of University College, London, against the Gresham Charter, we notice the following representatives of science and Fellows of the Royal Society:—Sir F. Abel, Prof. I. B. Balfour, Sir Henry Bessemer, H. S. Caxter, Sir J. N. Douglass, W. T. Thiselton-Dyer, Prof. W. H. Flower, Prof. E. Frankland, Dr. George Harley, R. B. Hayward, H. Hudleston, Prof. T. H. Huxley, Prof. E. Ray Lankester, Prof. Norman Lockyer, Prof. O. J. Lodge, Sir John Lubbock, Prof. D. Oliver, Prof. J. Prestwich, Prof. G. J. Romanes, Sir Henry Roscoe, Prof. Burdon Sanderson, J. Wilson Swan, Prof. Sylvester, E. B. Tylor, and Prof. W. F. R. Weldon. The protest contained equally influential names in the fields of literature, art, and politics; thus forming a document having no small weight in the final decision of the Government regarding this futile attempt to solve the problem of a Metropolitan University.

WE regret to have to record the death of Sir William Bowman, F.R.S., the eminent ophthalmic surgeon. He died of pneumonia at Joldwynds, his house near Dorking, on Tuesday last. He was born on July 20, 1816. In 1840 he was elected assistant surgeon at King's College Hospital, where he afterwards became full surgeon. He was also for a time assistant surgeon, and then full surgeon, at the Royal London Ophthalmic Hospital. He acted as the first president of the Ophthalmological Society of Great Britain, which he helped to found; and in 1884 he was created a baronet in recognition of his professional eminence. Sir William was a master of the various methods of ophthalmic surgery, and did much to improve them and to place them on a sound scientific basis. He held a leading place among those who made accessible to English students the knowledge obtained by the invention of the ophthalmoscope; and to him belongs the honour of having overcome the hostility